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Does the Village Fund Matter in Thailand?

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Abstract

This paper evaluates the impact of the Thailand Village and Urban Revolving Fund on household expenditure, income, and assets. The revolving fund was launched in 2001 when the Government of Thailand promised to provide a million baht (about $22,500) to every village and urban community in Thailand as working capital for locally-run rotating credit associations. The money —about $2 billion in total—was quickly disbursed to locally-run committees in almost all of Thailand's 74,000 villages and more than 4,500 urban (including military) communities. By May 2005, the committees had lent a total of about $8 billion, with an average loan of $466. Using data from the Thailand Socioeconomic Surveys of 2002 and 2004, each of which surveys almost 35,000 households, the authors find that the borrowers were disproportionately poor and agricultural. A propensity score matching model finds that Fund borrowing in 2004 was associated with, on average, 1.9 percent more income, 3.3 percent more expenditure, and about 5 percent more ownership of durable goods. These results are broadly consistent with the results from instrumental variables models (where the identifying instrument was the inverse of village size), which however show a smaller (marginal) effect. Households that borrowed both from the revolving fund and from the Bank of Agriculture and Agricultural Cooperatives gained substantially more in terms of higher income than those who borrowed from either one or the other or from neither.

This paper—a product of the Sustainable Rural and Urban Development Team, Development Research Group—is part of a larger effort in the department to understand the cost-effectiveness of rural financial institutions. Policy Research Working Papers are also posted on the Web at http://econ.worldbank.org. The author may be contacted at skhandker@worldbank.org.

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1. Introduction

In 2001, the government of Thailand launched the Thailand Village and Urban Revolving Fund (VRF) program, which aimed to provide a million baht (about $22,500) to every village and urban community in Thailand as working capital for locally-run rotating credit associations.[1]

Thailand has almost 74,000 villages and over 4,500 urban (including military) communities, so the total injection of capital into the economy envisaged by the "million baht fund" amounted to 78 billion baht, equivalent to about $1.75 billion, making it the most ambitious of the estimated 120,000 microfinance initiatives anywhere in the world.[2] The program was put into place rapidly. By the end of May 2005 the VRF committees had lent a total of 259 billion baht ($8.3 billion at the July 2007 exchange rate of Baht 31.23/$) to 17.8 million borrowers (some of whom borrowed more than once). This represents an average loan of $466. The total repayment of principal amounted to 168 billion baht, leaving outstanding principal of 91 billion baht.

In this paper we ask a narrowly focused question: Has the VRF had an impact on household incomes, spending, and asset accumulation, and, if so, how large are these effects? An answer to this question is necessary, but not sufficient, to help the Government of Thailand determine whether the program should be expanded or revised, and to help governments of other countries determine whether they should introduce or expand similar microcredit schemes. In order fully to address these policy issues, one would also need information on the costs of the program. A complete cost-benefit analysis of the Thailand Village Fund would be highly desirable, but goes beyond the scope of this paper.

The VRF represents a policy experiment on a grand scale, but it is not the only major source of household credit, even in rural areas. The Bank for Agriculture and Agricultural Cooperatives (BAAC) has an extensive network of rural lending. So it is appropriate to ask what additional role the VRF has played, an issue that we also address in this paper.

We summarize the relevant details of the VRF program in section 2, set out our general approach in section 3, describe the data employed in the impact evaluation in section 4, and in the subsequent sections explain the methodology and report the results of the impact evaluation using propensity score matching (section 5), instrumental variables (section 6), and panel data methods (section 7). The paper ends with a short set of conclusions in section 8.

[1] The average exchange rate during 2001 was Bht44.51/$, which implies that a million baht are equivalent to $22,468. The exchange rate as of mid-July 2007 was Bht31.23/$, which would value a million baht at $32,020; this is the exchange rate that we use throughout the rest of the paper.

[2] Estimated number of microfinance initiatives is from Kaboski and Townsend (2009), p.10.

2. The Thailand Village Revolving Fund

The Thailand Village Revolving Fund became operational very rapidly. Inaugurated in 2001, Village and Urban Community Fund Committees (henceforth "Village Fund Committees") had been formed in 92% of the villages and urban communities in Thailand by 2002, and much of the money had been disbursed. By May 2005, 99.1% of all villages had a Village Fund in operation and 77.5 billion baht, representing 98.3% of the originally scheduled amount, had been distributed to Village Fund Committees (Arevart 2005).

Although the initial working capital came from the central government, the Village Funds are locally run, and have some discretion in setting interest rates, maximum loan amounts, and the terms of loans; some require, or at least encourage, savings deposits as a condition for borrowing. The Village Fund Committees process loan applications; households borrow and repay with interest; and the money is lent out again. The Village Fund Committees do not handle money directly; this is done by a number of intermediaries, of which the most important are the Government Savings Bank (GSB), which operates mostly in urban areas, and the Bank for Agriculture and Agricultural Cooperatives (BAAC), which operates only in rural areas and semi-urban communities.

There are five steps that must be taken in order for a Village Fund to become operational:

(a). The village first sets up a local committee to run the fund and to determine the lending criteria (interest rate, loan duration, maximum loan size, and objectives).

(b) The properly-established committee then opens an account at the BAAC (which has about 700 branches) or another "facilitator", and the government deposits a million baht into the account.

(c) The local Fund committee sifts through loan applications and determines who may borrow and under what conditions (interest rate, duration, etc.).

(d) The borrowers go to the BAAC (or other facilitator) to get access to the loans. Each borrower must open an account – the minimum balance, if it is at the BAAC, is 100 baht – to which their loan is transferred.

(e) The borrower repays the loan with interest. This requires him or her to visit a BAAC branch (or that of another facilitator); the borrower typically deposits the repayment directly into the village fund account. The BAAC provides a regular listing of transactions to each Village Fund.

A number of rules govern the establishment and operating procedures of the committee: three quarters of the adults in the village must be present at the meeting where it is established; the committee should have about 15 members, half of them women; while there is some discretion about the amount lent per loan, it should not generally exceed 20,000 baht and should never exceed 50,000 baht; the loans must charge a positive interest rate; and it is recommended that loans have at least two guarantors.

The government rates Village Funds on a variety of efficiency and "social" criteria; in any given year, those that are rated AAA are provided with a "bonus" of a further Bht100,000 to add to their working capital.

In addition, Village Funds can borrow an additional million baht (or sometimes just half a million baht, see below) from the BAAC or other facilitator. The size of this additional loan - i.e. half a million, or a million baht - is determined by the BAAC using its own (banker's) criteria. Only Village Funds that are ranked 1st class or 2nd class by BAAC may borrow a million baht; the others (3rd class) may only borrow half a million baht. The BAAC says that about 1% of these loans are overdue. The BAAC thus rates the managerial efficiency and potential of VRF Associations and may be intending gradually to withdraw from micro-lending by giving these village funds a space for competition to run village banks. The BAAC recognizes that Village Fund Committees generally have an informational advantage in determining who is a good candidate for a loan.[3] Some of the more dynamic Village Funds are trying to become rural banks, which would potentially lead to an efficiency gain in that it would allow money to move from one village to another.

3. Measuring the Impact of the Village Revolving Fund

The injection of loanable funds due to the VRF was substantial, averaging 2.7% of annual income, or 7.1% of income for the 38% of households who borrowed. Because a million baht was available for every village, regardless of size, the importance of the VRF declined with village size: in the smallest tenth of villages, VRF loans represented 7.9% of income, but just 1.1% of income in the largest decile of villages (Table 1). What impact might one expect from such a sizeable one-time infusion of cash?

It is not self-evident that an injection of credit into a rural economy will have a measurable impact, or a positive impact. If financial markets operate well – information is cheap and readily available, there are no policy distortions – then households should already have access to as much credit as they can productively use, and they would mainly substitute VRF credit for other sources of credit. So for the VRF to have an impact on output, it must be predicated on the existence of market imperfections. As a general proposition, this is not unreasonable, as credit markets have well-known informational asymmetries that in turn can lead to the inefficient allocation of credit, excessive loan default, monopoly profits for well-informed lenders, and even credit market collapse (Bardhan and Udry 1999, p.91). The important point is that it cannot be assumed, a priori, that the VRF will necessarily have a major impact on household welfare.

According to the Socio-Economic Survey undertaken in 2004, 24% of respondent households said that they did not borrow from the VRF because they had no need for credit, and a further 25% said that they did not borrow from the VRF because they did not want to take on more debt. We have assumed that in the absence of general equilibrium effects, the introduction of the VRF credit cannot be expected to have an

[3] This process, however, could potentially squeeze out some existing borrowers who may have less access to BAAC loans, and yet not be able to get VRF loans for one reason or another. Moreover, some VRFs may be inefficient for the following reasons: (i) lending to unqualified borrowers; (ii) favoring committee members; (iii) extending loans that are larger than the limit (e.g. 50,000 baht); (iv) not insisting on repayment; (v) charging a lower interest rate; and (vi) landing for longer-than-allowed periods.

impact on the incomes or spending patterns of these households; however, this is not an innocuous assumption, because the very availability of easier credit may reduce the incentive for precautionary ("buffer stock") savings, and allow even non-borrowers to spend more than they otherwise would have.

Of those who did borrow, some may not have been credit-constrained, meaning that they had access to as much credit as they wanted, given the available price. They would then only have taken on VRF loans because they were cheaper. In part this would produce an income effect – substituting cheap for expensive credit – but the lower price of credit would also provide an incentive to borrow more overall. The effect could be large; one in six VRF borrowers said that they borrowed from another source to repay the VRF loan, and the average annual interest rate paid on those sources was 46.0%; given an average VRF interest rate of 6.0%, this represents a gain of 40%; given the mean loan size of 16,183 baht, the interest saving would be equivalent to 4.9% of an average borrowing household's annual income. While this probably an upper bound on the cost savings from VRF borrowing, it is enough to allow non-interest consumption for borrowers to rise by at least 6.1%, with no change in household income.

Other VRF borrowers may have been credit-constrained, in the sense that they already wanted to borrow more at the available price of credit. Presumably existing lenders were reluctant to lend more due to prudential concerns, which in turn may have been justified, or may have resulted from asymmetric information. It is entirely possible that the village-level VRF would, in many cases, have better knowledge about the ability of village households to service loans than most outside lenders, and thus improve the efficiency with which credit is allocated.

We do not have direct evidence on whether VRF loans substituted for other credit, or supplemented other borrowing. Kaboski and Townsend (2009), based on a rural sample of 800 households, find evidence that in 2003, households took on VRF loans without reducing their other borrowing. This sits well with the view that many households are credit-constrained, but of course is not inconsistent with the case of non-constrained households responding to lower borrowing costs.

Much microlending is seen as desirable because it allows households to invest more, and so raise their earnings, and certainly the VRF was originally viewed as a vehicle for promoting the development of non-farm enterprise. In this case the impact goes from loan to more investment to more income to more consumption. On the other hand, many households use credit for consumption purposes – to smooth consumer spending over the course of a year, or make a lumpy purchases (including durable goods), or increase consumer spending now relative to in the future. In this case one would observe an increase in consumer spending without a corresponding rise in income. Given that households are heterogeneous, and only some would borrow from the VRF for productive purposes, our presumption is that the VRF will have a stronger impact on consumption spending than on income.

Whether VRF loans were used for investment or for consumer spending, the effect is likely to be complicated by the fact that a number of credit schemes are already in place. In rural areas, the most

important is the Bank for Agriculture and Agricultural Cooperatives (BAAC), which practices individual as well as group-based lending (mainly to support farming), mobilizes savings as part of financial intermediation, and is widely considered to be a successful rural finance institution (Yaron 1992, Fitchett 1999). Therefore it is legitimate to wonder whether the VRF has an added value to rural households that the BAAC could not provide – are they substitutes or complements? In other words, the relative effectiveness of both programs is an issue worth examining from the policy point of view, an issue to which we return in section 5.

In short, our main task is to measure the impact of the VRF program on three outcome variables of interest:

- **Expenditure per capita.** The measure of expenditure available is based on the Socioeconomic Surveys of 2002 and 2004, and includes 56 categories of expenditure (and home production), including the rental value of housing, but does not include the rental value of the household's durable goods or vehicles (for lack of data).
- **Income per capita.** This measure includes 24 categories of income, and includes the rental value of housing (but not of durable goods).
- A number of measures of **household assets**, including whether the household has a washing machine, a VCR, or a motorized vehicle. The SES-2004 did not collect information on the total value of household assets.

But now we are faced with a methodological problem: VRF borrowers do not represent a random sample of the households (or adults) surveyed in the Socioeconomic Survey of 2004 – among other things, they are poorer and more rural.

To get around the problem of non-random assignment, we are obliged to turn to a number of econometric techniques. These include propensity score matching (section 5) and instrumental variables (section 6), using data from the Thailand Socioeconomic Surveys of 2002 and 2004. These surveys also included a panel of rural households, which allows us to estimate the impact of the VRF using double differences, and instrumental variables with household fixed effects (section 7). But before discussing the impact evaluation techniques and results, some additional description of the data is in order.

4. The Data

The data for the impact evaluation come from the Thailand Socioeconomic Surveys of 2002 and 2004. The 2004 survey interviewed 34,843 households (covering 116,444 people) throughout the country drawn from 2,044 municipal "blocks" and 1,596 villages in 808 districts. The data were collected in four rounds, spread throughout the year. The survey collected a wide variety of socio-economic data, including relatively detailed information on household income and expenditure. It used stratified random sampling with clustering; all the

descriptive results presented in this paper apply the appropriate weights (unless otherwise indicated). The 2002 survey used substantially the same questionnaire and covered 34,785 households.

An interesting feature of these two surveys is that they include a panel of 5,755 rural households. An effort was made in 2004 to re-survey all 6,309 households that had been surveyed in rural areas in rounds 2 and 3 of the 2002 socioeconomic survey. This represents an annual attrition rate of 4.5%, which is relatively low. A comparison between panel households and those who dropped out of the panel found no appreciable differences in the relevant variables (in 2002), allaying concerns about attrition bias.

The summary statistics in Table 2 come from a special module that was included in the 2004 socioeconomic survey and that asked all adult members of households about their experience with the VRF. By 2004, a sixth of all adults had borrowed at least once from the VF, with higher proportions of borrowers among the poor (defined as those in the poorest quintile, as measured by expenditure per capita) and among those in rural areas; in this respect, VRF lending differs sharply from the older "village bank" programs in Northeast Thailand analyzed by Coleman (2002), where the bulk of the loans, and gains, accrued to the wealthier villagers. Adults in 38 percent of households had borrowed from the VRF by 2004.

Of those adults who did *not* borrow, less than one percent had been refused a VRF loan, although a further 4% thought that they would be turned down. On the other hand, over a quarter of non-borrowing adults said they had no need to borrow, and almost a third said that they did not want to go into debt. Poor households were less likely to indicate that they did not need to borrow, but more likely to be fearful to going into debt.

The average amount borrowed in the most recent VRF loan was 16,183 baht (about $518), and this was only slightly less than the amount requested on average. The mean interest rate charged on VRF loans was 6.0 percent per year, but there was considerable variation, as Figure 1 shows: substantial numbers of Village Funds charged annual interest rates of 5, 3, or 12 percent. The interest rate paid by poor, or rural, borrowers was essentially the same, or perhaps slightly lower, than that paid by other adults.

Although the rhetoric surrounding the Village Revolving Fund program emphasized the importance of providing finance for processing and packaging, over half of all VRF borrowers said that they planned to use the money for relatively traditional agricultural purposes. This effect was even more marked among poor and rural borrowers. Borrowing is fungible, so this does not necessarily imply that spending on agricultural activities actually rose as a result of the implementation of the Village Fund program, but there is a dissonance between the reported uses of the borrowed funds and the original aspirations for the Fund.

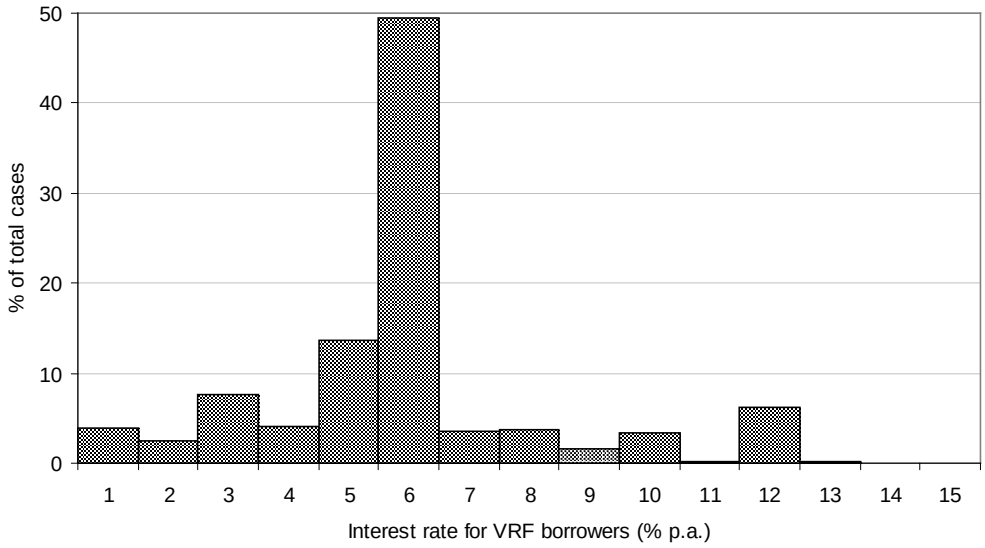

Figure1. Interest Rates Charged by Village Funds, 2004

Source: Thailand Socio-Economic Survey 2004

Eight percent of VRF borrowers reported that they were overdue on repayments, and the proportions were similar for poor, and for rural, households. However, a sixth of those who obtained VRF credit in turn borrowed elsewhere in order to repay their VRF loan. The interest rates charged by those alternative sources of credit were high, averaging 46 percent (on an annualized basis).

Despite the challenges that some faced in repaying the VRF loans, seven out of ten borrowers said that their economic situation had "improved" as a result of the program and just 2 percent said that it had worsened. However, less than a third of borrowers said that the VRF system should be left unchanged; substantial numbers wanted the loan amounts to be larger (34% of respondents), longer (34%), cheaper (37%), or to be focused more on the poor (25%).

In 2004, women were slightly less likely than men to have borrowed from the Village Fund: 15.5% of adult women borrowed from the fund, compared to the overall average of 16.6%. Women asked for, and received, slightly smaller loans; paid a slightly higher interest rate; and were less likely to borrow to buy agricultural inputs or equipment. However, in most other respects, female borrowers are indistinguishable from male borrowers, as may be seen by comparing the first and last columns of numbers in Table 2.

A 2005 survey undertaken in the northeast of Thailand by the Thailand Development Research Institute (TDRI) found that about 40% of households had borrowed from the VRF, and among those who borrowed, slightly over 90% said they were satisfied with the process. There is, however, anecdotal evidence that in some cases the injection of credit has led villagers to borrow too much, leading to difficulties when the

funds had to be repaid (Laohong 2006, Gearing 2001). There have also been reports of corruption in the administration of the VRF in some scores of villages.

The most rigorous study to date of the impact of the VRF uses data from the 2003 and earlier rounds of a panel of 960 households that Robert Townsend and his colleagues have been following for a number of years in four provinces of Thailand; 800 households were followed throughout 1997-2003, and this is the sample used in the study by Kaboski and Townsend (2009). Although the sample size is relatively small, the survey is rich in detail on household financial assets and transactions. Their most striking finding is that the proportion of household credit coming from "formal" sources (including the VRF) jumped from 37% in 2001 to 69% in 2002, and was accompanied by little reduction in the use of other credit; in other words, at least as of 2003, VRF credit supplemented rather than replaced existing sources of credit.

Although the VRF is widely used, and reported levels of satisfaction with it are high, this is no guarantee that it has had a measurable impact on the outcome variables of interest. Some critics have argued that many VRF borrowers view the money more as a grant than a loan, in which case it might be expected to lead to a one-time increase in per capita expenditure and the value of household durables, but not raise income. Defenders argue that the VRF has had an effect on productivity, raising income and, via higher income, boosting expenditures. Yet others argue that the main effect of the VRF has been to substitute for other sources of credit, with very little net impact on real output, spending, or welfare. To determine the truth in these arguments, a formal impact evaluation is required.

5. Propensity Score Matching

Our first approach to measuring the impact of the VRF is by creating a quasi-experimental design that matches VRF borrowers with "otherwise identical" non-borrowers, and quantifies any difference in outcome variables between these two groups. Formally, let

X_i be a vector of pre-treatment covariates (such as age of head of household, location of household, and so on),

Y_{i0} be the observed value of the outcome variable (such as expenditure) in the absence of the treatment,

Y_{i1} be the observed value of the outcome variable for household i if it has been treated (i.e. it has borrowed from the VRF), and

T_i be the treatment (equal to 1 if the household is treated, to 0 otherwise).

We want to measure $\tau_i \equiv Y_{i1}-Y_{i0}$, but this is impossible, because an individual is either in the treatment group (so be observe Y_{i1}) or the comparison group (so we observe Y_{i0}), but never in both. If we are willing to assume that households are "assigned" randomly to the treatment group, once we have conditioned on the covariates, then by a proposition first established by Rubin (1977), the average treatment effect $(\tau|_{T=1})$ is

identified and is equal to $\tau|_{T=1,X}$ averaged over the distribution of $X|_{Ti=1}$. In other words, we can measure the average impact of the VRF by taking each borrower, finding an identical non-borrower (conditioned on the *X* covariates), computing the difference in the outcome variable of interest, and taking its mean.

This procedure would only be straightforward if there were just a few covariates; in practice the problem is more tractable if we can create a summary measure of similarity in the form of a propensity score. Let $p(X_i)$ be the probability that unit *i* be assigned to the treatment group, and define

$$p(X_i) \equiv \Pr(T_i = 1 \mid X_i) = E(T_i \mid X_i). \quad (1)$$

In practice, this probability – the propensity score – could be estimated using a logit or probit equation. Rosenbaum and Rubin (1983) show that conditional independence extends to the propensity score, so that treatment cases may be matched with comparison cases using just the propensity score. Furthermore, the average treatment effect may be obtained by computing the expected value of the difference in the outcome variable between each treated household and the perfectly matched comparison household (as matched using the propensity score). Perfect matching is not possible in reality, so in practice one needs to compute

$$\hat{\tau}|_{T=1} = \frac{1}{|N|}\sum_{i \in N}\left(Y_i - \frac{1}{|J_i|}\sum_{j \in J_i} Y_j\right), \quad (2)$$

where Y_i is the observed outcome for the *i*th individual who is treated and J_i is the set of comparators for *i*. The comparators may be chosen with replacement – the approach we take – in which case the bias is lower but the standard error higher than without replacement. We use single nearest neighbor matching, whereby one chooses the closest comparator, although other approaches are possible (Abadie et al. 2001); Dehejia and Wahba (2002) argue that the choice of matching mechanism is not as crucial as the proper estimation of the propensity scores.

Broadly following an algorithm outlined by Dehejia and Wahba (2002), we first estimated propensity scores by applying a probit model to a limited number of covariates. We then sorted the observations by propensity score and divided them into strata sufficiently fine to ensure that there was no statistically significant difference in propensity scores between treated and non-treated households within each stratum. We confined this comparison to the area of "common support" – where the propensity scores of the treated and untreated overlap – and typically needed between 15 and 21 strata. We then checked for the "balancing property," which means that within each stratum we tested (using a 1% significance level) whether there was a difference in the covariates between the treated and non-treated group. Our initial propensity score models were not well balanced, so we added covariates (including dummy variables for Thailand's 76 provinces) and we were able to generate models that were adequately balanced. For instance, when we confined our sample to rural areas, the propensity score model had 101 covariates, generated 13 strata, and produced 14 cases where covariates were not balanced. This is acceptable, given that at a one percent level of statistical significance one would expect to find, erroneously, about 13 cases of imbalance (false negatives).

A listing of the variables used in estimating the propensity scores for 2004 is given in Table 3 (except for the provincial dummy variables). The first thing to note is that on average VRF borrowers are substantially poorer than those who do not borrow from the VRF, whether measured by monthly expenditure per capita (2,549 baht vs. 4,286 baht) or income per capita (3,209 baht vs. 6,088 baht), or by access to subsidized medical care (93% have a 30 baht medical card, vs. 77% for non-borrowers). Compared to non-borrowers, those who borrow from the VRF are more than twice as likely to be farmers and to be self-employed, they are more likely to live in the Northeast region, they have larger families, and there are more earners per household. The important point here is that borrowers differ appreciably from non-borrowers, at least unless one conditions on the covariates.

The estimate of the probit propensity score equation for the full sample is also shown in Table 3. The equation fits well enough and, as noted above, appears to be adequately balanced. One of the more influential variables is the inverse of the number of households per village (or block): The Thai Village Fund initially provided a fixed amount to every village, irrespective of size, which means that households living in a large village are less likely to have access to these loans than those in a small village. This effect shows clearly in the estimates of the propensity score equation reported in Table 3.

Basic Results

Given the propensity scores, it is then possible to match each treatment case with a nearby comparison case, and hence to estimate the impact of VRF borrowing. The results are summarized in Table 4; the upper half of the table refers to 2004 (with separate propensity score equations for the full sample, for rural households only, and for the panel), and the bottom half to 2002.

When propensity score matching is used with the full sample of households surveyed in 2004, VRF borrowing is associated with a statistically significant 3.3% more expenditure per capita and a not-quite-significant 1.9% higher income per capita. Translated into average increases (at the mean) this implies a rise in per capita spending of 84 baht per month and of income of 61 baht per month. A reasonable interpretation is that VRF loans are partly, but not exclusively, functioning as consumer credit; they also appear to be working through the effect on income. The results based on the 2002 data are comparable: VRF borrowing is associated with a 3.1% rise in income (t=1.90) and a 2.6% rise in expenditure (t=2.15). To put these numbers into perspective, the mean size of a VRF loan was 16,183 baht (Table 2), and mean monthly income per person was 4,987 baht (Table 1) in 2004.

The increases reported in Table 4 are plausible. The boost to income in 2004 represents an annualized rate of return of 4.5% on the amount borrowed (which averaged 16,183 baht). However, these effects are only found when expenditure (or income) per capita is shown in log form; when measured in levels, the VRF has no statistically significant impact in these cases. The use of the log of income (rather than

its level) puts more emphasis to increases for poorer households, as the proportional effects (i.e. logs) are given more weight in these cases. To explore this further, we divided households into quintiles based on the levels of expenditure per capita, and then applied propensity score matching (with a single nearest neighbor) to each category. The striking result, shown in Table 5, is that the impact of VRF borrowing is only strong for the poorest quintile, a finding that holds both for 2002 and 2004. It would thus be appropriate to categorize the VRF policy as "pro-poor."

It is instructive to breakdown the impacts further, for each major category of income; the results are shown in rows 8-14 in Table 7. More VRF borrowing is associated with more farm income (up 49%, albeit from a modest base of just 522 baht per capita per month) and more income from non-farm enterprises (up 26%). On the other hand, VRF borrowing is not associated with higher wage or transfer income.

One may also break down the impacts by consumer expenditure category (see rows 15-26 in Table 7). There are substantial increases in spending on grain and meat, and also on vehicle operation, although this last effect is not quite statistically significant. None of the other measured impacts are statistically significant. These results differ somewhat from those reported by Kaboski and Townsend (2009, Table 5), who found, for a sample of villages in central Thailand, that VFR credit raised spending on alcohol, and on repairs to homes and vehicles.

The VRF appears to have the biggest impact in rural areas. If the analysis is repeated for rural households only, the effect is a statistically significant 6.9% boost to expenditure and 4.3% increase in income in 2004 (upper panel of Table 4), although the comparable effects in 2002 were much smaller.

There are minimal gender effects. For households that reported having a male head, VRF borrowing was associated with a 5.2% rise in expenditure and 4.8% increase in income. These figures are only marginally higher than those for female-headed households, where expenditure rose 5.0% and income by a (non-significant) 2.8%, as shown in rows 31 and 32 of Table 7.

In addition to the effect on income or expenditure, it might also be expected that VRF borrowing would have an effect on the accumulation of household assets. It is not possible to measure household gross or net assets using the Socioeconomic Survey data, but there is a listing of the major physical assets, of which some of the most important are given in Table 6. There we see, for instance, that 64% of all households surveyed had a phone in 2004; the rate was 59% for VRF borrowers and 67% for non-borrowers. We then used our propensity-score matching and found that, for instance, phone ownership among VRF borrowers was 5.4 percentage points higher than among comparable non-borrowers. Similar effects were found for VCRs, fridges, washing machines, and motorized transport. This, coupled with the smaller impact on income than on expenditure, suggests that VRF borrowing was used to some extent in order to get improved access to consumer and producer durables, despite the fact that fewer than 2% of households reported that this was the ostensible purpose of their VRF borrowing (see Table 2).

Robustness

How robust are these findings? A number of useful checks are summarized in Table 7: row 1 shows the basic result from Table 4, which is a 3.3% increase in expenditure per capita. Using the same propensity score equation we first measured the sensitivity of the results to alternative matching methods. Most of the results are of the same order of magnitude: stratification matching (i.e. matching within broader strata) shows a 4.2% impact of VRF borrowing on expenditure; kernel matching, which compares the treated case with all neighbors, but with high weights for near neighbors, shows an impact of between 1.8% (Gaussian kernel) and 4.5% (Epanechnikov kernel). Only caliper matching gives a radically different result – it compares all treated cases (i.e. VRF borrowers) to those with a propensity score within a radius of 0.001 – indicating, implausibly, that VRF borrowing reduced expenditure by 18%. This may be because a substantial number of borrowers with high propensity scores were not matched, and so were excluded, because there were no comparators in the immediate vicinity. However, this result does lead one to question the assertion by Dehejia and Wahba (2002) that the choice of matching mechanism is of secondary importance.

A somewhat different check on the robustness of our results is to match treatment households with non-treatment comparators using *direct* nearest neighbor matching rather than first estimating propensity scores. It is not clear that direct ("covariate") matching represents an improvement, even in principle, over propensity-score matching, and it is computationally intensive, but if both approaches give similar results then one can have more confidence in the conclusions. The results, for households living in rural areas (and using dummy variable for regions, rather than provinces) are shown in rows 6 and 7 of Table 7 and show that while VRF borrowing is associated with a statistically significant 7.6% increase in per capita spending as measured using propensity score matching; the effect is much smaller using direct matching – an increase of 1.3% if the direct match is based on a single nearest neighbor – and not statistically significant.

We also re-estimated the results after deleting villages that were either very small (under 50 households) or rather large (with at least 500 households). Perhaps surprisingly, the results are somewhat stronger, and show a 4.8% increase in expenditure and 3.7% rise in income due to the VRF (lines 27 and 28 in Table 7).

The most important maintained assumption in propensity score matching is that "the process by which individuals are assigned or assign themselves to treatment" is ignorable (DiPrete and Gangl 2004). That is, after removing the effects of observable variables, we may proceed as if subjects were randomly assigned to treatment. This is a strong assumption. In practice there are likely to be unobserved variables, such as motivation or ability, that simultaneously affect the outcome, and the assignment to treatment. By definition we cannot quantify the effects of this "hidden bias". One solution, proposed by Rosenbaum (2002), is to test the sensitivity of the results to the introduction of a hypothetical "confounding variable" *W*, that affects the

odds of being assignment to treatment. Let π_i be the probability that unit *i* receives treatment, and $\boldsymbol{X}_i$ the observed covariates. Then the log odds ratio is given by

$$\ln\left(\frac{\pi_i}{1-\pi_i}\right) = \kappa(\mathbf{X}_i) + \gamma U_i, \qquad 0 \le U_i \le 1.$$

Under the hypothesis of ignorability, $\gamma=0$ (or equivalently, $\Gamma=1$, where $\Gamma \equiv e^{\gamma}$). With higher values of Γ, propensity score matching will be less precise. Rosenbaum shows how to obtain significance levels (using a Wilcoxon sign-rank test) and new confidence intervals (if the treatment effects are additive) for different values of Γ. The results of estimating these Rosenbaum bounds for the log of expenditure per capita are shown in Table 8, and show that our results are not especially robust; if an unobserved variable were to cause the odds ratio of treatment assignment to vary by a factor of about 1.07, then our finding of a impact would no longer be statistically significant at even the 10% level. Although our results are sensitive to the assumption of ignorability, this potential loss of significance is, as DiPrete and Gangl (2004) rightly point out, just a worst-case scenario.

Our final robustness check is to estimate the effect of borrowing on income and consumption using a common impact model (Haughton and Khandker 2009). Let Y_i measure the outcome, $\boldsymbol{X}_i$ be a vector of covariates, T_i measure the treatment under consideration (i.e. VRF borrowing), and ε_i represent a zero-mean error term, and estimate

$$Y_i = \alpha_C + \alpha_{TC} T_i + \mathbf{X}_i \beta + \varepsilon_i, \qquad i = 1, \ldots, n \quad \text{observations.}$$

Then the estimate of the coefficient α_{TC} should be able to measure the impact of the borrowing. The results are shown in Table 9, and use the same other covariates as in the propensity score matching (see Table 3). When the treatment is measured as a binary variable, set equal to 1 if the household borrows from the VRF, then the impact is to raise expenditure per capita by 2.3% - broadly in line with the 3.3% impact as measured using propensity score matching (Table 4); however, the estimated effect on income is nil. One may also measure the impact of the amount of borrowing, *given that one is a borrower*. The middle section of Table 8 shows that an additional 100 baht of borrowing is associated with 83 baht more spending and 143 baht more income, figures that are on the high side, particularly for income, unless "lumpiness" in investment is a commonly binding constraint. The estimates in the bottom panel of Table 8 imply that a 10% higher loan is associated with 1.4% more consumption or 1.8% more income, again rather large effects. The common impact model is less compelling than propensity score matching because it does not confine the estimates to a region of common support, and does not try to tailor comparisons for each treated case.

The Bank for Agriculture and Agricultural Cooperatives

The VRF is not the only, or even necessarily the most important, source of credit for Thai households. The Bank for Agriculture and Agricultural Cooperatives has an extensive network of rural lending. Of the households covered by the 2004 socioeconomic survey, 23% borrowed from the VRF only, 15% borrowed from both the VRF and BAAC, and 6% borrowed from the BAAC only. These figures differ slightly from those presented earlier because they only refer to the two most important loans incurred by any given household. But the fact that many households borrow both from the VRF and the BAAC raises the possibility that our earlier results may be picking up the effect of BAAC borrowing and attributing it to VRF borrowing.

We therefore applied our propensity score matching approach to borrowing from the BAAC, and report the results in Table 10. For each comparison (i.e. row in Table 10) we estimated separate propensity-score equations. From Table 10 it is clear that those who borrowed from the BAAC in 2004 were comparably poor to, and somewhat more dependent on farm income than, VRF borrowers.

The first point to note is that, based on the results of the propensity-score matching analysis set out in Table 10, borrowing from the BAAC, with or without other loans, is associated with substantially higher expenditure per capita (+6.5%) and income per capita (+6.1%). This effect is larger than that of borrowing from the VRF (expenditure per capita rises 3.3%, income per capita by 1.9%, as shown in Table 4).

The most striking finding is that the *combination* of borrowing from the BAAC and VRF has particularly powerful effects, and is associated with 9.1% higher expenditure and 8.5% higher income. Loans from these two sources appear to be complementary. A plausible interpretation is that many households, particularly farm households, are credit constrained, even if they borrow from the BAAC; the VRF, by relaxing these constraints, enables them to boost their incomes. It is noteworthy that borrowing from the BAAC but not VRF, or from the VRF but not BAAC, has a small and only marginally significant effect on expenditure levels and an even weaker effect on incomes. This hints at a real, but moderate, degree of "lumpiness" in investment, where the full return on using borrowed money is only obtained when the sum is large enough.

The propensity score matching results appear, on balance, to show that VRF borrowing raised household income and expenditures on average, and that much of the productive effect operated in agriculture. In the next section we use a different approach, instrumental variables, further to check the robustness of these results.

6. Instrumental Variables

We are interested in finding an unbiased estimate of the impact effect – an estimate of γ – in an outcome equation of the form

$$Y_i = \alpha + \gamma.T_i + \mathbf{X}_i\beta + \varepsilon_i, \quad i = 1,...,n \qquad (3)$$

where Y_i is the outcome of interest, T_i is a dummy variable that equals 1 if the household borrows from the VRF, and the $\boldsymbol{X_i}$ variables are relevant covariates. However, T_i is a "troublesome explanator" (Murray 2005) because it is likely correlated with ε_i: as the basic numbers in Tables 2 and 4 show, VRF borrowers are not a random sample of the population – they are poorer, spend less, and own fewer durable goods.

An unbiased estimate of γ may be found if one can construct an adequate participation ("first stage") equation of the form

$$T_i = f(\mathbf{Z}_i, \mathbf{X}_i), \qquad (4)$$

where the instruments $\boldsymbol{Z_i}$ should be strongly correlated with T_i ("instrumental relevance") yet be uncorrelated with ε_i ("instrument exogeneity"). Then the estimated value, $\hat{T}_i$, is used in place of T_i in equation (3).

We may think of the instrumental variables (IV) estimate of γ as reflecting the "marginal" impact of the treatment; that is, it measures the impact on expenditure (or income) of one more person borrowing from the VRF. This differs from the propensity score matching measure, which quantifies the average impact across all those who are treated. If treatment brings diminishing marginal returns, one might expect the impact, as measured using propensity score matching, to be larger than that measured using the instrumental variables approach.

The main practical problem with the IV approach is finding appropriate instruments, yet "the credibility of IV estimates rests on the arguments offered for the instruments' validity" (Murray 2005, p.11). In our case there is one good candidate: the inverse of the size of the village. A feature of the VRF is that it provided a million baht to each Village Rotating Fund, irrespective of the size of the village. Thus the probability of obtaining a VRF loan ("participation") is approximately in inverse proportion to the size of the village. Our measure of the size of the village is the number of households, which is likely to be closely correlated with the theoretically ideal measure (the number of people eligible for VRF loans, which is the number of adults aged 20 and above).

The IV estimates of the impact of the VRF are summarized in Table 11. In each case the first step equation is probit; an example, for the log of expenditure per capita, is shown in detail in Appendix Table A1. In all cases the influence of the instrument in the first-stage equations is highly statistically significant, clearly showing its relevance.

The second-stage equation is linear. Where possible, estimation was done using maximum likelihood and using sampling weights; in the cases when this estimator did not converge we used a simpler two-step

procedure on the unweighted data. In all cases the reported z-statistics have been adjusted to account for the fact that one is using $\hat{T}_i$ rather than T_i in the outcome equation.

The IV results in Table 11, for 2004, show a positive but not statistically significant impact of the VRF on expenditure and income. In rural areas the measured effects are negative. Curiously, the impact on farm income, and on non-farm income, are separately large and positive. The results for 2002 show that VRF borrowing raised expenditure by 9.9%, and income by 8.4%, although the latter effect is not highly statistically significant.

These results are not particularly robust. The middle rows of Table 11 show the effect on the IV estimates of adding other instruments. The first instrument is "anydebt", which equals 1 if the household in 2004 has any outstanding debt. This is strongly correlated with whether a household borrows from the VRF, but weakly associated with the outcome variable (e.g. simple correlation with expenditure per capita of -0.035; weighted correlation of -0.109). The inclusion of this instrument raises the measured impact of the VRF to a statistically significant 16% for income and 20% for expenditure, levels that are certainly on the high end.

It might be objected that "anydebt" is not exogenous, if households borrow from the VRF when they would not otherwise have borrowed. Alternatively one could use a measure of "non-VRF debt", set equal to 1 if the household has debt other than VRF debt. With this instrument the measured impact of VRF borrowing on household spending rises to an implausible 46%, but even here it might be argued that the new instrument is not truly exogenous.

Finally, we also add, as an instrument, the interest rate charged by the VRF. It is plausible that a higher interest rate would deter borrowing – indeed, the weighted correlation coefficient is -0.054 – and be essentially unrelated to the log of expenditure per capita (correlation of 0.046). The inclusion of this instrument raises the measure of the impact of VRF borrowing to unrealistically high levels. But it is by no means a fully satisfactory instrument: when it is included, the sample size falls, because interest-rate information is only available for villages that have an operating VRF.

In sum, the results of our IV analysis are not very sharp and are partly contradictory. It does seem reasonable to conclude, however, that the most satisfactory models just use the inverse of the village size as an instrument; and in this case, the marginal impact of the VRF on expenditure and income is minimal. Combined with the propensity score matching results, it appears that the VRF raises spending and income on average, but is experiencing diminishing returns at the margin.

Our results are broadly in line with those found by Kaboski and Townsend (2006), who also used an instrumental variables approach, but with data from the 2003 and earlier rounds of a panel of 960 households surveyed in four rural provinces. They checked for robustness by applying a variety of econometric specifications (levels, changes, and estimates with and without outliers). Their main findings are that greater use of the VFR was associated with somewhat higher levels of household expenditure, and perhaps an

increase in income, and with an increase in agricultural investment as well as spending on fertilizer and pesticides.

Kaboski and Townsend also found that VRF borrowing was associated with an apparent reduction in net household assets. This might seem surprising, but could be due to mismeasurement (a farmer might have invested in drainage or field leveling, and this might not be picked up in survey questions), or because better access to credit reduces the need to hold assets, or because households overborrowed.

7. Panel Data

An effort was made, in the socioeconomic survey of 2004, to re-survey half of the rural households that had been interviewed in 2002. This produced a panel of 5,755 rural households for which information is available for both years. The panel data allow us, in principle, to get a less biased measure of the impact of VRF borrowing, because one can eliminate unobserved variable bias, provided that the bias is linear and does not vary over time. It also helps that the introduction of the VRF was a "surprise", in the sense that it was proposed and implemented swiftly, and households in 2002 could not easily adjust their behavior in anticipation of future lending.

Double Differencing

The simplest way to use the panel data is by double differencing. If, before the borrowing, income Y_i depends on covariates $\boldsymbol{X}_i$, then

$$Y_{i,t0} = a + c.\mathbf{X}_{i,t0} + \varepsilon_{i,t0}, \qquad (5)$$

and afterwards

$$Y_{i,t1} = a + b.T_i + c.\mathbf{X}_{i,t1} + \varepsilon_{i,t1}. \qquad (6)$$

with $\varepsilon_{it} = \eta_i + \mu_{it}$. Differencing gives

$$Y_{i,t1} - Y_{i,t2} = b.T_i + c.(\mathbf{X}_{i,t1} - \mathbf{X}_{i,t0}) + \mu_{i,t1} - \mu_{i,t2}. \qquad (7)$$

Considering those households that did not borrow from the VRF in 2002, a regression of the differenced outcome variable on the treatment variable (which equals 1 for those who borrowed from the VRF in 2004) and the change in the covariates should estimate the impact, while "sweeping away" the effects of unobservable or mismeasured (but time-invariant) covariates. The results of this exercise are shown in the middle panel of Table 12, which shows little to no impact of the VRF on expenditure (impact of 2.0% but t-statistic of 1.04), income, or even farm or non-farm income.

To check the robustness of these results, before computing the double differences we first estimated the propensity scores using the 2002 data and the same variables as in Table 3, and then confined the double

differencing to the area of common support. We weighted the differences for each treated case (i.e. VRF borrower) by 1, and each comparison case by $p/(1-p)$ – where p is the propensity score – as recommended by Imbens (2004; also Ravallion 2006). The results, shown in the bottom panel of Table 12, were similar to those of the unweighted, unconstrained estimates: there is a hint of an impact on expenditure per capita, and on non-farm income per capita, but none of the effects are statistically significant at the conventional levels.

It is also possible to confine the double differencing to those who did not borrow in 2004 (and look at the effects of borrowing in 2002); or to those who did borrow in 2002 (and look at the effects of continuing to borrow in 2004).[i] None of the results in these cases (not shown here) were statistically significant.

Panel Instrumental Variables

As a final exercise we undertook an instrumental variables analysis using the (rural) panel data and incorporating household fixed effects. The linear first-stage equation uses, as instruments, the presence of a VRF in the village, this measure multiplied by the educational level of the household head, and the size of the village multiplied by the educational level of the head. The full equations, for the case where the outcome is the log of expenditure per capita and the comparison is between those who borrowed from the VRF in 2004 and those who borrowed neither in 2002 nor in 2004, are shown in Appendix Table A2.

The key results are summarized in table 13. Households that borrowed from the VRF in 2004 had 15% more income and 18% more expenditure than those who borrowed in neither year, holding other influences constant; these increases are statistically significant, but also rather large. If, instead, the comparison is between those who borrowed both in 2002 and 2004 and those who borrowed only in 2004, the impact of the second year of borrowing was to raise income by 8% and spending by 10%. These statistically significant rises are within the bounds of plausibility.

8. Conclusions

This study of the impact of the Thailand Village Fund is based entirely on data from the socio-economic surveys of 2002 and 2004, undertaken just one and three years after the VRF was launched. In the absence of random assignment, we were obliged to use quasi-experimental methods to quantify the effect of the VRF on outcome variables. The propensity score matching approach generates reasonable results: the Village Revolving Fund does appear to have had an impact, raising expenditures by 3.3% and income by 1.9% in 2004. These results are tolerably robust to most specifications of matching, and we may interpret these numbers as reflecting the average impact of the VRF program.

By and large the other results – based on using instrumental variables on cross-section data, double differences, and instrumental variables using a rural panel – do not contradict the propensity score matching results. The instrumental variables estimates suggest that the *marginal* impact of the VRF may be small, even though, based on the propensity score matching, the *average* impact is more substantial. The double difference results show little effect, but the instrumental variables analysis with household fixed effects shows a surprisingly large impact of VRF borrowing in rural areas.

Our interpretation of these findings is that the VRF has indeed had a moderate impact on household spending, and also (but to a lesser extent) on household income; this is consistent with our expectations, based on theory.

Further investigation shows a number of interesting patterns. First, most of the effect of VRF borrowing is concentrated in the poorest quintile of the population (as measured by expenditure per capita), where it raised spending by 5.2%, making the program markedly pro-poor. Second, the effect of the VRF appears to work most convincingly through its influence on farm income, suggesting that it is credit-constrained farmers who have best been able to put the loans to productive use. This is not what the designers of the Fund had envisaged; instead they had expected that it would boost household-level non-farm enterprise (and there is some, if modest, evidence of this too). We speculate that the short-term nature of the VRF loans makes them suitable for farmers – they allow for the financing of inputs during a crop cycle – but are not sufficiently long-term (or perhaps large) enough to be very useful for most of the other remunerative activities that households might initiate.

The third interesting finding is that there are synergies between VRF and BAAC loans; borrowing from one or the other alone has only a modest discernible impact on incomes or even expenditure, in contrast to the large impact associated with borrowing from both sources. This has some important practical implications. The BAAC should be slow to withdraw from village-level lending, even if it is tempted to do so by a perception that the VRF can fill the gap; or alternatively, the BAAC should be sure to channel enough resources via the VRF to allow it to fill the gap adequately. Our results also suggest that if the government wants to expand the VRF, the most productive approach would be to target poorer farming communities.

Finally, a caveat. Our results do not allow one to make a judgment about the desirability of the VRF. That would require additional information about the full costs of the program and an evaluation of its sustainability. It would also be valuable to determine whether the impact of the VRF weakens over time, a finding that is common elsewhere (e.g. Chen et al. 2006; Khandker 2005). These both require further research, which would be particularly desirable given the importance of the Thai experiment with large-scale microcredit.

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Table 1. Village Rotating Fund Lending by Village Size

Decile	# of households	Income in 2004, baht per month		Average VRF loan/income	% of households borrowing from VRF
		Per capita	Per household		
1	75	3,588	12,975	7.9	63.6
2	103	4,163	14,450	4.6	49.3
3	119	5,342	18,836	2.5	37.0
4	133	5,012	17,145	2.6	37.8
5	147	5,431	18,537	2.1	35.7
6	163	5,317	18,312	2.1	34.6
7	183	5,006	17,385	2.1	35.7
8	211	4,966	16,889	2.1	35.6
9	256	5,071	17,487	1.7	30.5
10	368	6,120	20,020	1.1	21.3
Total	175	4,987	17,198	2.7	38.2

Source: From Thailand Socio-Economic Survey of 2004.

Table 2. Summary of Use of Village Fund, by Adults, 2004

		All	Poorest fifth*	Rural	Female
Number of observations (unweighted)		80,950	13,180	30,892	43,916
Expenditure per capita (baht/month)		3,398	1,060	2,578	3,427
Income per capita (baht/month)		4,717	1,455	3,345	4,745
Did you obtain at least one VRF loan since 2002? (% saying yes)		16.6	20.0	21.5	15.5
Why did you not obtain a loan?	Number of observations	69,486	10,820	24,547	38,035
	Applied but was refused (%)	*0.7*	*1.1*	*0.8*	*0.7*
	No need (%)	*28.5*	*16.0*	*25.1*	*28.7*
	Believed would be refused (%)	*4.1*	*4.4*	*3.9*	*3.9*
	Too expensive (%)	*0.2*	*0.4*	*0.2*	*0.2*
	Did not find guarantors (%)	*0.9*	*1.1*	*0.7*	*0.8*
	Did not like to be in debt (%)	*29.5*	*37.8*	*33.1*	*29.7*
	Don't know about VRF (%)	*7.7*	*3.1*	*2.6*	*7.7*
	Other (%)	*28.0*	*36.1*	*33.4*	*28.0*
	VRF is not available (%)	*0.5*	*0.0*	*0.1*	*0.4*
How much money did you ask to borrow in this loan? (Baht)		17,183	18,236	17,438	16,340
How much did you actually borrow in this loan? (Baht)		16,183	17,312	16,462	15,322
Annualized interest rate on the VRF loan (%)		6.0	5.8	5.9	6.1
What was the main (true) objective for obtaining this loan	Number of observations	11,250	2,354	6,298	5,881
	Buy agricultural equipment/inputs (%)	*39.5*	*44.9*	*42.2*	*35.3*
	Buy animals (for sale/use) (%)	*9.7*	*12.3*	*10.4*	*8.4*
	Buy agricultural land (%)	*1.7*	*1.6*	*1.8*	*1.7*
	Buy non-farm business equipment/inputs (%)	*10.3*	*3.6*	*8.9*	*11.6*
	Business construction (%)	*3.6*	*1.3*	*3.0*	*4.2*
	Buy consumer durables (%)	*1.4*	*2.0*	*1.3*	*1.6*
	Improve dwelling (%)	*4.8*	*4.3*	*4.4*	*4.6*
	School fees (%)	*4.0*	*2.1*	*3.4*	*4.7*
	Health treatment (%)	*0.6*	*0.7*	*0.6*	*0.9*
	Ceremonies (%)	*0.2*	*0.2*	*0.2*	*0.2*
	On-lending (%)	*0.8*	*0.7*	*0.8*	*0.9*
	Other (%)	*23.4*	*27.1*	*23.0*	*25.6*
	Not reported (%)	*0.2*	*0.1*	*0.2*	*0.2*
Were you overdue in repaying this loan? (% saying yes)		7.7	7.9	7.5	7.9
Did you borrow from somewhere else in order to repay this loan? (% saying yes)		16.1	18.9	16.6	16.8
What rate of interest did you have to pay on this other loan? (% per annum)		46.0	44.2	43.9	49.6
How did this loan change your economic situation	*Improved (%)*	*71.1*	*70.9*	*71.7*	*70.9*
	Unchanged (%)	*27.0*	*27.2*	*26.4*	*27.0*
	Worsened (%)	*1.9*	*2.0*	*1.9*	*2.2*
Why was your loan application refused?	Number of observations	249	62	96	123
	No funds left (%)	*39.1*	*40.5*	*43.7*	*32.5*
	Application incomplete (%)	*8.2*	*8*	*8.6*	*5.2*
	No guarantors (%)	*19.2*	*19.8*	*14.9*	*20.8*
	Other (%)	*30.9*	*31.6*	*30.4*	*40.0*
	Not reported or unknown (%)	*2.6*	*0.1*	*2.5*	*1.5*
If refused, did you obtain a loan from other sources instead? (% saying yes)		45.0	38.7	46.7	52.6
How should the VRF system be changed? (% mentioning item)	*No changes needed*	*30.2*	*28.3*	*31.5*	*30.4*
	No guarantors	*13.4*	*12.5*	*12.3*	*13.1*
	Higher loan amounts	*33.6*	*36.7*	*36.3*	*33.1*
	Longer repayment periods	*33.9*	*40.8*	*38.2*	*33.4*
	Lower interest/grants	*36.9*	*40.9*	*38.5*	*37.1*
	Repayment in kind	*4.9*	*6.5*	*5.5*	*5.0*
	Should give money only to the poorest	*25.2*	*22.3*	*21.5*	*25.6*
	Other	*6.7*	*5.2*	*5.3*	*6.8*

Source: Thailand Socioeconomic Survey 2004.
Note. Unit of observation is an adult (aged 20 or older). Sampling weights were used in all cases. * Poorest quintile as measured by expenditure per capita.

Table 3. Summary of Variables Used in Propensity Score Analysis for 2004

	Full sample		VRF borrowers		Propensity Score Equation	
	Mean	*Std. Dev.*	Mean	*Std. Dev.*	Coefficient	*p-value*
Does household borrow from VRF? (Yes=1)	0.38	*0.49*	1.00	-		
Age of head (in years)	49.67	*14.84*	50.37	*13.16*	0.017	0.00
Educational level of head (in years)	7.09	*4.39*	6.09	*3.18*	0.100	0.00
Head of household is male (yes=1)	0.70	*0.46*	0.74	*0.44*	-0.048	0.05
Number of adult males in household	1.09	*0.71*	1.17	*0.71*	-0.153	0.00
Number of adult females in household	1.27	*0.70*	1.33	*0.63*	-0.136	0.00
Number of males working in agriculture	0.45	*0.65*	0.68	*0.70*	-0.042	0.42
Number of males working in industry	0.20	*0.46*	0.17	*0.43*	-0.113	0.03
Number of males working in trade	0.13	*0.39*	0.10	*0.34*	-0.241	0.00
Number of males working in services	0.20	*0.44*	0.15	*0.39*	-0.095	0.07
Number of females working in agriculture	0.44	*0.60*	0.69	*0.64*	0.053	0.30
Number of females working in industry	0.17	*0.42*	0.15	*0.39*	-0.118	0.02
Number of females working in trade	0.13	*0.39*	0.11	*0.35*	-0.196	0.00
Number of females working in services	0.21	*0.48*	0.15	*0.39*	-0.127	0.01
Municipal area (yes=1)	0.33	*0.47*	0.12	*0.33*	-0.452	0.00
Province 1 (metro Bangkok)					-0.660	0.00
province2					-0.238	0.06
province3					-0.173	0.154
…						
Age of household head (in years '00), squared	2,688	*1,556*	2,710	*1,381*	-1.935	0.00
Educational level of head (in years), squared	69.55	*83.77*	47.18	*54.97*	-0.006	0.00
One-person household	0.10	*0.31*	0.04	*0.20*	-0.257	0.00
Household with two parents	0.67	*0.47*	0.75	*0.43*	0.097	0.00
Household with one parent	0.10	*0.30*	0.09	*0.29*	-0.074	0.03
Household has 30 baht medical card	0.83	*0.38*	0.93	*0.26*	0.223	0.00
Household gets lunch or food subsidy	0.24	*0.43*	0.34	*0.48*	0.068	0.00
Size of household	3.45	*1.66*	3.84	*1.61*	0.100	0.00
Head of household is self-employed	0.48	*0.50*	0.65	*0.48*	-2.146	0.00
Head of household is an employee	0.34	*0.47*	0.23	*0.42*	-2.351	0.00
Head of household has another employment	0.18	*0.39*	0.12	*0.33*	-2.211	0.00
Number of earners in household	1.94	*1.07*	2.21	*1.03*	0.247	0.00
1/(number of households per village or block)	0.00694	*0.0031*	0.00775	*0.0034*	29.810	0.00
Constant					0.395	0.57
Memo: Outcome variables						
Household current income, baht/capita per mth	4,987	*7,119*	3,209	*3,385*	Pseudo R2	0.190
Household consumption, baht/capita per mth	3,622	*4,190*	2,549	*2,410*	Region of	0.004 to
Household farm income, baht/capita per month	522	*1,809*	785	*2,048*	common	0.985
Household non-farm income, baht/capita per mth	3,964	*6,780*	2,065	*2,855*	support	
Percentage rise in income since 2002	0.55	*15.09*	0.32	*16.32*		
Number of observations	*34,843*		*10,985*		34,752	

Source: Thailand socioeconomic survey, 2004.
Note: Means are weighted to take structure of sampling into account.

Table 4. Propensity Score Matching results

	Expend-iture per capita	Ln(exp-enditure per capita)	Income per capita	Ln(income per capita)
	(1)	(2)	(3)	(4)
2004				
		Means		
Whole sample	3,622	7.88	4,987	8.08
VRF borrowers	2,549	7.63	3,209	7.79
Not VRF borrowers	4,286	8.04	6,088	8.26
		Matched comparisons		
Full sample				
VRF-not VRF	-36.4	**0.033**	-228.0	**0.019**
t [n=10,957]	-0.59	2.67	-2.32	1.27
Rural households only				
VRF-not VRF	48.0	**0.069**	-10.0	**0.043**
t [n=6,051]	0.55	3.79	-0.09	1.98
Panel households only				
VRF-not VRF	59.2	**0.043**	68.7	**0.056**
t [n=2,459]	0.45	1.44	0.39	1.59
2002				
		Means		
Whole sample	3,131	7.75	4,446	7.94
VRF borrowers	2,044	7.46	2,660	7.61
Not VRF borrowers	3,529	7.85	5,102	8.06
		Matched comparisons		
Full sample				
VRF-not VRF	-28.94	**0.026**	-205.92	**0.031**
t [n=10,957]	-0.65	2.15	-2.33	1.90
Rural households only				
VRF-not VRF	10.4	**0.031**	-242.0	**0.012**
t [n=6,051]	0.20	1.93	-2.04	0.58
Panel households only				
VRF-not VRF	103.0	**0.073**	-2.5	**0.071**
t [n=2,459]	1.44	2.83	-0.02	2.13

Source: Based on data from Thailand Socioeconomic Surveys of 2002 and 2004.
Notes: * Minimum values of ln(farm income per capita) and ln(non-farm income per capita) were set equal to 0.

Table 5. Propensity Score Matching by Quintile for ln(expenditure per capita)

	2004		2002	
	VRF – not VRF	t-statistic	**VRF – not VRF**	t-statistic
Effects by expenditure per capita quintile				
Quintile 1 (poorest)	**0.052**	4.87	**0.036**	3.59
Quintile 2	**0.007**	1.56	**0.004**	0.84
Quintile 3	**-0.005**	-1.33	**-0.005**	-1.17
Quintile 4	**0.007**	1.42	**-0.009**	-1.37
Quintile 5 (richest)	**-0.044**	-1.92	**-0.047**	-1.81

Source: Based on data from Thailand Socioeconomic Surveys of 2002 and 2004.

Table 6. The Effect of VRF Borrowing on Household Durable Assets, Based on the Propensity Score Matching Model

		Sample means		**Matched comparisons**	
	Whole sample	VRF borrowers	Non-VRF borrowers	VRF - non VRF	t-statistic
2004					
HH has VCR	0.60	0.61	0.60	**0.036**	4.04
HH has fridge	0.80	0.82	0.78	**0.045**	6.56
HH has washing machine	0.36	0.33	0.39	**0.049**	5.36
HH has phone	0.64	0.59	0.67	**0.057**	6.54
HH has motorized transport	0.74	0.84	0.68	**0.047**	6.66
HH uses Internet	0.18	0.12	0.21	**0.003**	0.42
2002					
HH has VCR	0.38	0.40	0.31	**0.018**	1.81
HH has fridge	0.76	0.76	0.77	**0.060**	7.14
HH has washing machine	0.29	0.32	0.22	**0.020**	2.05
HH has phone	0.40	0.45	0.26	**0.020**	2.01
HH has motorized transport	0.70	0.67	0.79	**0.048**	5.58
HH uses Internet	0.03	0.04	0.00	**-0.006**	-2.71

Source: Based on data from the Thailand Socioeconomic Surveys of 2002 and 2004.

Note: For 2004, the number of treatment households is 10,957 and the propensity score equation is based on a total sample of 34,843; for 2003 there were 7,238 treatment households (i.e. who borrowed from the VRF) out of a total sample of 34,785 households. The sample means are weighted to reflect the sampling design.

Table 7. Robustness Checks for Propensity Score Matching Results

		VRF – not VRF	t-statistic	# treated
	Propensity Score Matching, full data set using provincial dummies			
	Impact on ln(expenditure per capita)			
1	Nearest neighbor (base case)	**0.033**	2.67	10,957
2	Stratification matching	**0.042**	5.47	10,957
	Kernel matching			
3	A: Gaussian	**0.018**	2.51*	10,957
4	B: Epanechnikov	**0.045**	5.67*	10,957
5	Radius (caliper) matching, radius = 0.001	**-0.182**	-22.59	10,884
	Rural dataset using regional dummies			
6	Propensity Score Matching	**0.076**	4.39	6,051
7	Direct ("Covariate") Matching	**0.013**	1.02	
	Breakdown by sources of income			
8	Ln(expenditure per capita)	**0.033**	2.67	10,957
9	Ln(income per capita)	**0.190**	1.27	10,957
10	Ln(wage income per capita)	**0.095**	1.41	10,957
11	Ln(non-farm enterprise income/capita)	**0.256**	3.87	10,957
12	Ln(farm income/capita)	**0.493**	8.87	10,957
13	Ln(transfer income/capita)	**0.050**	0.93	10,957
14	Ln(other income/capita)	**0.133**	3.20	10,957
	Breakdown by consumption category			
15	Grain	**7.69**	4.48	10,957
16	Dairy	**-0.27**	-0.15	10,957
17	Meat	**10.07**	3.29	10,957
18	Alcohol (consumed at home)	**2.06**	0.84	10,957
19	Alcohol (consumed outside home)	**-3.23**	1.05	10,957
20	Fuel	**-1.88**	-0.71	10,957
21	Tobacco	**1.97**	1.31	10,957
22	Ceremonies	**9.47**	1.28	10,957
23	Home furnishings	**1.15**	1.06	10,957
24	Vehicle operation & maintenance	**12.42**	1.51	10,957
25	Clothes	**2.37**	0.44	10,957
26	Education	**-1.79**	-0.58	10,957
	Excluding villages with <50 and ≥500 households			
27	Ln(expenditure per capita)	**0.048**	3.97	10,818
28	Ln(income per capita)	**0.037**	2.52	10,818
	Male head of household			
29	Ln(expenditure per capita)	**0.052**	3.55	7,918
30	Ln(income per capita)	**0.048**	2.71	7,918
	Female head of household			
31	Ln(expenditure per capita)	**0.050**	2.31	3,039
32	Ln(income per capita)	**0.028**	1.11	3,039

Source: Based on data from Thailand Socio-Economic Surveys of 2002 and 2004.
Note: * Based on bootstrapped standard errors with 100 replications.

Table 8. Rosenbaum Bounds for the Impact of VRF Borrowing on the Log of Expenditure per Capita, Based on Propensity Score Matching, Thailand 2004

		Confidence Interval	
Γ	Critical p-value	Lower Bound	Upper Bound
1	0.0000	0.021	0.045
1.025	0.0004	0.013	0.054
1.05	0.0123	0.004	0.062
1.075	0.1185	-0.003	0.070
1.10	0.4437	-0.011	0.078

Notes and Sources: Based on data from Thailand Socioeconomic Survey of 2004. For definition of Γ, see text, and also DiPrete and Gangl (2004); Γ=1 assumes no hidden bias. Estimation used the `rbounds` command in Stata written by Markus Gangl. The estimated impact (see Table 3) was 0.033, which implies that VRF borrowing was associated with a 3.3 percent increase in expenditure per capita.

Table 9. Estimates of Treatment Effects from Common Impact Model

Outcome variable	Measure of treatment	Coefficient	t-statistic	p-value	Adj. R^2	Observations
Ln(expenditure/capita)	Borrows from VRF	0.023	3.7	0.00	0.56	34,752
Ln(income/capita)	Borrows from VRF	0.0002	0.0	0.98	0.58	34,752
Expenditure/capita	VRF borrowing	0.827	8.4	0.00	0.24	10,735
Income/capita	VRF borrowing	1.425	10.5	0.00	0.30	10,735
Ln(expenditure/capita)	Ln(VRF borrowing)	0.144	16.6	0.00	0.46	10,735
Ln(income/capita)	Ln(VRF borrowing)	0.175	17.0	0.00	0.48	10,735

Source: Based on data from Thailand Socio-Economic Survey of 2004.

Table 10. Propensity Score Matching Results for BAAC vs. VRF

2004	Expenditure per capita		Income per capita		# observations (unweighted)
	Level (1)	Log (2)	Level (3)	Log (4)	(7)
			Means		
Whole sample	3,622	7.88	4,987	8.08	34,843
Borrow from VRF	2,549	7.63	3,209	7.79	10,985
Borrow from VRF but not BAAC	2,724	7.68	3,396	7.83	7,268
Borrow from BAAC	2,378	7.58	3,107	7.75	5,624
Borrow from BAAC but not VRF	2,656	7.66	3,463	7.82	2,854
Borrow from BAAC and VRF	2,292	7.56	2,934	7.73	3,717
Borrow from neither VRF nor BAAC	4,486	8.09	6,390	8.31	21,951
			Matched Comparisons		
	Level	Log	Level	Log	
Borrow from BAAC (and possibly others)					
BAAC-not BAAC	124.16	**0.065**	25.79	**0.061**	34,843
t-statistic	1.88	4.46	0.22	3.40	
Borrow from BAAC (but not from VRF)					
BAAC-not BAAC	45.95	**0.036**	-29.01	**0.038**	34,843
t-statistic	0.41	1.63	-0.14	1.40	
Borrow from VRF (but not from BAAC)					
VRF-not VRF	9.38	**0.021**	-98.67	**0.015**	34,843
t-statistic	0.15	1.67	-1.05	0.97	
Borrow from TVC and BAAC					
BAAC+VRF-not BAAC or VRF	190.4	**0.091**	150.2	**0.085**	34,843
t-statistic	3.0	5.8	1.8	4.5	
2002	Expenditure per capita		Income per capita		# observations
	Level	Log	Level	Log	
			Means		
Whole sample	3,130	7.75	4,446	7.94	34,785
Borrow from VRF	2,044	7.46	2,660	7.60	7,243
Borrow from VRF but not BAAC	2,111	2.48	2,733	7.62	4,760
Borrow from BAAC	2,018	7.43	2,724	7.59	5,326
Borrow from BAAC but not VRF	2,098	7.44	2,911	7.61	2,843
Borrow from BAAC and VRF	1,942	7.43	2,547	7.58	2,483
Borrow from neither VRF nor BAAC	4,486	8.09	6,390	8.31	21,951
			Matched Comparisons		
	Level	Log	Level	Log	
Borrow from BAAC (and possibly others)					
BAAC-not BAAC	125.79	**0.061**	131.11	**0.098**	34,785
t-statistic	2.68	4.42	1.06	5.26	
Borrow from BAAC (but not from VRF)					
BAAC-not BAAC	112.12	**0.043**	99.10	**0.057**	34,785
t-statistic	1.84	2.54	0.59	2.49	
Borrow from VRF (but not from BAAC)					
VRF-not VRF	-178.54	**-0.027**	-378.17	**-0.028**	34,785
t-statistic	3.37	-1.97	-3.15	-1.57	
Borrow from TVC and BAAC					
BAAC+VRF-not BAAC or VRF	122.9	**0.071**	8.6	**0.077**	34,785
t-statistic	2.3	4.2	0.1	3.3	

Source: Based on data from Thailand Socioeconomic Surveys of 2002 and 2004.

Notes: * Minimum value of ln(farm income per capita) and ln(non-farm income per capita) set equal to 0 in all cases. All the propensity score equations use dummy variables for the provinces, as well as the other variables

listed in Table 2.

Table 11. Instrumental Variable Estimates Using Data for 2004 and 2002

	Expenditure per capita	Income per capita	Farm income per capita	Non-farm income per capita
	(1)	(2)	(3)	(4)
2004		**Means**		
Whole sample	3,622	4,987	522	3,964
Borrow from VRF	2,549	3,209	785	2,065
Do not borrow from VRF	4,286	6,088	360	5,140
		Impacts (in log form)		
Instrument: nhinv				
Full sample, two-step estimator	**0.016**	**0.017**	**0.49**	**0.43**
z-statistic (n=34752)	0.36	0.33	2.47	3.60
Rural sample, two-step estimator	**-0.237**	**-0.082**	**1.778**	**-0.057**
z-statistic (n=12858)	-4.89	-1.46	8.28	-0.36
Checks for robustness				
Instruments: nhinv, anydebt				
Full sample, maximum likelihood estimator	**0.196**	**0.163**	**0.952**	**0.176**
z-statistic (n=34752)	15.54	10.78	18.79	5.73
Instruments: nhinv, anydebt				
Rural sample, two-step estimator	**0.172**	**0.129**	**0.957**	**0.128**
z-statistic (n=12858)	9.81	6.2	12.29	2.15
Instruments: nhinv, non-VRF debt				
Full sample, maximum likelihood estimator	**0.464**			
z-statistic (n=34752)	24.2			
Instruments: nhinv, non-VRF debt, interest rate				
Full sample, two-step estimator	**0.620**	**0.543**		
z-statistic (n=26930)	15.3	12.01		
2002		**Means**		
Whole sample	3,131	4,446	466	824
Borrow from VRF	2,044	2,660	746	508
Do not borrow from VRF	3,529	5,102	364	940
		Impacts (in log form)		
Instrument: nhinv.				
Full sample, two-step estimator	**0.099**	**0.084**	**0.310**	**-0.183**
z-statistic (n=34759)	2.55	1.69	1.37	-1.04
Rural sample, two-step estimator	**-0.176**	**-0.098**	**0.002**	**0.181**
z-statistic (n=13209)	-3.30	-1.42	-0.01	0.52

Source: Based on data from Thailand Socioeconomic Surveys of 2002 and 2004.

Notes: nhinv is the inverse of the number of households per village. Non-VRF debt is a binary variable that is equal to 1 if a household has debt that is not from the VRF. anydebt equals 1 if a household has debt from any source, and is 0 otherwise. The equations for the full-sample two-step estimator using nhinv, for 2004, are shown in Appendix 1.

Table 12. Double Difference Estimates of Impact of TVF Borrowing Using Rural Panel Data for 2002 and 2004

	Expenditure per capita	Income per capita	Farm income per capita	Non-farm income per capita
	(1)	(2)	(3)	(4)
	Means, 2002 (in 2004 prices)			
Full panel sample	2,370	3,128	710	432
Households that borrowed in 2004, not in 2002	2,300	3,203	678	429
Households that do not borrow in 2002 or 2004	2,529	3,381	582	459
	Impacts (in log form)			
Unweighted, using full panel data*				
Impact	**0.020**	**0.003**	**-0.075**	**0.034**
t-statistic	1.04	0.15	-1.24	0.34
Sample size	3,335	3,335	1,370	652
Weighted, using data in area of common support**				
Impact	**0.021**	**-0.000**	**-0.041**	**0.111**
t-statistic	1.18	-0.01	-0.69	1.15
Number of positive observations	3,327	3,327	1,368	650

Source: Based on panel component of Thailand Socioeconomic Surveys of 2002 and 2004 (which covers rural areas only).
Notes: * Coefficient from regression of log change in value (e.g. income) on borrowing, controlling for the log changes in the same covariates as in Table 3. The full panel has 5,054 observations, but only households that did not borrow in 2002 were included in the estimation; missing or negative values further reduced the estimation sample slightly. ** As for the unweighted results, except that in this case a propensity score (p) was first estimated and used to confine the comparison to the area of common support, and then to weight the comparison cases by p/(1-p).

Table 13. Instrumental Variables Estimates Using Rural Panel Data for 2002 and 2004

	Expenditure per capita	Income per capita	Farm income per capita
	(1)	(2)	(3)
Panel data sample:		**Means, 2002 and 2004**	
Whole sample	2,457	3,179	714
Borrow from VRF in 2002 only	2,499	2,951	621
Borrow from VRF in 2004 only	2,376	3,179	712
Borrow from VRF in 2002 and 2004	2,259	2,904	928
Borrow from VRF in neither 2002 nor 2004	2,632	3,413	575
		Impacts (in log form)	
VRF borrowing in 2004 vs. no VRF borrowing*			
IV panel estimate	**0.179**	**0.152**	**0.459**
z-statistic	3.19	2.24	1.93
VRF borrowing in 2004 vs. VRF borrowing in both years			
IV panel estimate	0.096	0.082	0.315
z-statistic	2.91	2.02	1.95

Source: Based on (rural) panel components of Thailand Socioeconomic Surveys of 2002 and 2004.
Note: * Hausman test of systematic difference in coefficients between IV and OLS: $\chi^2(24)=2.99$, for a probability > 0.9999. Full regression results are shown in Appendix 2. Identifying instrument is the inverse of the number of households per village.

Appendix Table A1. Estimates for Instrumental Variables Equations (Probit first-stage and Linear second-stage) for 2004 for ln(expenditure per capita), Using nhinv as an Instrument*

	First-stage equation (probit)		Second-stage equation (linear)	
	Mean	***Std. Dev.***	**Mean**	***Std. Dev.***
Does household borrow from VRF? (Yes=1)			0.016	0.36
Age of head (in years)	0.017	4.85	0.023	19.82
Educational level of head (in years)	0.100	13.53	0.031	11.72
Head of household is male (yes=1)	-0.048	-1.95	0.007	0.80
Number of adult males in household	-0.153	-6.73	0.048	6.14
Number of adult females in household	-0.136	-7.04	0.059	8.78
Number of males working in agriculture	-0.042	-0.81	-0.014	-0.82
Number of males working in industry	-0.113	-2.14	0.075	4.34
Number of males working in trade	-0.241	-4.50	0.134	7.60
Number of males working in services	-0.095	-1.79	0.128	7.41
Number of females working in agriculture	0.053	1.04	-0.032	-1.93
Number of females working in industry	-0.118	-2.27	0.054	3.15
Number of females working in trade	-0.196	-3.79	0.120	7.05
Number of females working in services	-0.127	-2.53	0.133	8.24
Municipal area (yes=1)	-0.452	-25.54	0.092	10.35
Province 1 (metro Bangkok)	-0.660	-6.59	0.389	13.03
province2	-0.238	-1.88	0.246	6.37
province3	-0.173	-1.42	0.370	9.90
…				
Age of household head (in years '00), squared	0.000	-5.71	0.000	-19.30
Educational level of head (in years), squared	-0.006	-16.53	0.001	9.25
One-person household	-0.257	-6.84	0.290	24.71
Household with two parents	0.097	3.36	-0.040	-4.18
Household with one parent	-0.074	-2.25	0.016	1.46
Household has 30 baht medical card	0.223	9.01	-0.222	-28.23
Household gets lunch or food subsidy	0.068	3.22	-0.107	-14.04
Size of household	0.100	11.51	-0.134	-41.14
Number of earners	0.247	5.27	-0.010	-0.63
Head of household is self-employed	-2.146	-4.02	-0.199	-1.34
Head of household is an employee	-2.351	-4.40	-0.296	-1.98
Head of household has another employment	-2.211	-4.14	-0.153	-1.02
1/(number of households per village or block)	29.810	10.47		
Constant	0.395	0.73	7.630	50.04
Memo items				
Wald χ^2 (203)			50544	*p=0.00*
Λ	0.004	*0.15*		

Source: Thailand socioeconomic survey, 2004.
Note: nhinv is the inverse of the number of households in the village.

Appendix Table A2. Estimated Equations for the Panel Instrumental Variables Analysis for ln(expenditure per capita), VRF Borrowers in 2004 vs. Non-Borrowers

	First-stage equation			Second-stage equation		
	Coeffi-cient	t-statistic	p-value	Coeffi-cient	t-statistic	p-value
Did household borrow from VRF? (Yes=1)				**0.179**	**3.19**	**0.001**
Age of head (in years)	0.010	1.59	0.113	0.011	1.81	0.070
Educational level of head (in years)	0.048	3.53	0.000	0.002	0.20	0.840
Head of household is male (yes=1)	-0.120	-3.13	0.002	0.044	1.16	0.245
Number of adult males in household	0.030	1.31	0.189	-0.065	-2.94	0.003
Number of adult females in household	-0.012	-0.55	0.582	-0.161	-7.99	0.000
Number of males working in agriculture	-0.135	-2.28	0.022	-0.170	-2.96	0.003
Number of males working in industry	-0.068	-1.12	0.262	-0.121	-2.06	0.039
Number of males working in trade	-0.094	-1.49	0.138	-0.110	-1.82	0.069
Number of males working in services	-0.090	-1.54	0.124	-0.041	-0.72	0.469
Number of females working in agriculture	-0.100	-1.72	0.086	-0.119	-2.11	0.035
Number of females working in industry	-0.121	-2.00	0.046	-0.044	-0.75	0.455
Number of females working in trade	-0.083	-1.32	0.186	-0.091	-1.51	0.131
Number of females working in services	-0.039	-0.66	0.508	-0.056	-0.99	0.320
Age of household head (in years '000), squared	-0.032	-0.58	0.560	-0.107	-2.02	0.043
Educational level of head (in years '000), squared	-0.015	-0.03	0.980	0.312	0.54	0.590
One-person household	-0.042	-0.96	0.336	0.313	7.51	0.000
Household with two parents	-0.012	-0.31	0.756	-0.144	-3.94	0.000
Household with one parent	-0.085	-2.25	0.025	-0.014	-0.37	0.710
Household has 30 baht medical card	0.082	3.07	0.002	-0.089	-3.38	0.001
Household gets lunch or food subsidy	0.107	5.44	0.000	-0.027	-1.31	0.192
Head of household is self-employed	0.997	2.65	0.008	0.424	1.16	0.247
Head of household is an employee	0.958	2.55	0.011	0.418	1.14	0.253
Head of household has another employment	0.925	2.47	0.014	0.392	1.08	0.282
Number of earners in household	0.111	1.98	0.047	0.136	2.52	0.012
Village size × Educational level of head × 1000	-0.128	-2.96	0.003			
Is there a VRF in village? (yes=1)	0.364	10.17	0.000			
VRF in village × Educational level of head	-0.005	-0.90	0.367			
Constant	-1.668	-4.23	0.000	7.312	18.88	0.000
Memo items:						
Hausman χ^2					5.03	1.00
Number of observations	6,674					
F-test of coefficients	17.93	*(27,3308)*	0.00			
Wald χ^2 test				3,007,000	*(25)*	0.00
F-test that u_i=0	0.79	*(3338,3308)*	1.00	*3.10*	(3338,3310)	0.00
R^2: within / between / overall	0.127	*0.016*	0.042	0.099	*0.186*	0.168

Source: Thailand Socioeconomic Surveys of 2002 and 2004.

[i] From the panel of 5,054 households, we have the following breakdown of the number of households:

	Did not borrow from VRF in 2004	Borrowed from VRF in 2004
Did not borrow from VRF in 2002	2,291	1,048
Borrowed from the VRF in 2002	303	1,412